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HOW AND WHY DOES SHOPLIFTING IN THE VICINITY OF A RETAIL STORE AFFECT ITS SALES GROWTH?

ABSTRACT

Shoplifting imposes well-documented operational costs on retailers, yet its effects on store-level sales performance remain unquantified. This gap matters: if shoplifting deters shoppers, its economic burden extends far beyond inventory shrinkage. We examine whether shoplifting incidents in a retail store's vicinity affect its sales growth, under what conditions, and through what mechanisms. We propose a sequential demand-side pathway: shoplifting elevates shoppers' safety concerns, reducing shopper traffic, and suppressing sales growth. The Routine Activity Theory proposes two store characteristics that can mitigate this negative effect. First, the store's offline revenue share generates internal guardianship through heightened staff presence and physical monitoring. Second, temporal agglomeration generates external guardianship through co-present shoppers across neighboring stores during shared consumption windows. The empirical analysis uses a store-month panel of 70,330 branded retail stores across 27 U.S. cities from January 2019 through January 2026, drawing on 495,973 shoplifting incidents. Results confirm that shoplifting significantly reduces store sales growth. At the median store, local shoplifting is associated with a monthly sales loss of 36% of median monthly store sales and an annualized aggregate loss exceeding $2.15 billion. Stores with higher offline revenue share or greater temporal agglomeration experience meaningfully smaller reductions. Shopper traffic mediates approximately 59% of the total effect, confirming demand-side deterrence as a pathway. The evidence contributes to retail literature by documenting how and why shoplifting affects a store's performance. We offer managers two levers to mitigate this effect.

Keywords: shoplifting, store sales growth, shopper traffic, routine activity theory, guardianship, offline revenue share, temporal agglomeration

INTRODUCTION

U.S. retailers reported $112.1 billion in total inventory losses in FY 2022, of which 36% (the largest single source) were attributed to external theft, a category that encompasses shoplifting (National Retail Federation, 2023). *Shoplifting* is the act of stealing goods from a retail store without paying for them (Federal Bureau of Investigation, 2011). It includes concealing merchandise and leaving the store, switching price tags, and returning stolen items for refunds or store credit. Shoplifting is a form of theft and thus illegal in most places. Penalties can range from fines to jail time, depending on the value of the items and local laws. Common store responses to shoplifting include locking merchandise behind cases, reducing store hours, and increasing loss-prevention expenditures (McArdle, 2022), resulting in a retail landscape in which shoplifting, even when not directly witnessed by shoppers, may affect how they perceive, evaluate, and ultimately visit stores. Whether and how these conditions translate into a store's performance is a question the marketing discipline has not yet answered.

Store sales growth, the quintessential measure of retail performance, reflects a store's ability to sustain and expand its shopper base over time (Feng et al., 2017; Tuli et al., 2010; Whitler et al., 2018). If shoplifting incidents deter shoppers from visiting nearby stores, its impact extends well beyond the merchandise taken, strengthening the case for mitigation. Therefore, establishing whether and how shoplifting affects store sales growth is theoretically important for understanding retail performance and practically relevant to retailers and policymakers grappling with elevated crime in retail markets.

Prior research has examined shoplifting along two streams. The first has documented its operational costs, establishing it as a significant contributor to retail inventory shrinkage, and has cataloged the loss-prevention strategies retailers employ in response (Hollinger & Davis, 2006). The second has examined how store environments, safety-related cues, and perceived crime risk suppress patronage intentions even without direct victimization (Baker et al., 2002; Grewal et al., 2003; Kajalo & Lindblom, 2010). Most recently, store-level evidence from mass shooting events shows that proximate safety threats suppress retail sales and foot traffic through demand-side channels (Chiong et al., 2025). However, shoplifting differs from mass-shooting events in its frequency, ubiquity, and the subtler and more chronic nature of the safety signal it transmits. It is a routine feature of the retail environment, not a discrete high-salience incident. Whether shoplifting incidents translate into meaningful performance consequences at the store level remains an open question. Figure 1 situates the present study relative to prior work along two dimensions: type of crime (routine shoplifting vs. acute events) and the outcome examined (operational vs. demand-side financial outcomes).

Figure 1. Positioning the Current Research in Relevant Literature

	Routine crime *(shoplifting)*	**Acute crime** *(mass shootings, violence)*
Operational outcomes	Hollinger and Davis (2006); Korgaonkar et al. (2021) - Documents shrinkage costs and loss-prevention tactics - Supply-side focus only	Baker et al. (2002), Grewal et al. (2003) - Patronage intentions under perceived crime risk - No causal financial evidence
Demand and financial outcomes	*This study* *- Causal evidence to link routine shoplifting with store sales growth* *- Mechanism: shoppers' safety concerns and traffic deterrence* *- Heterogeneity explainers: offline revenue share, temporal agglomeration*	Chiong et al. (2025) - Mass shooting events suppressing retail revenue and foot traffic - Demand-side channel confirmed - High-salience, discrete events only

We address these research gaps by asking three questions: (1) whether shoplifting in a retail store's vicinity affects its sales growth (the *what*), (2) under what store-level conditions this effect varies (the *when*), and (3) through what mechanisms the effect operates (the *why*). We hypothesize that shoplifting in a retail store's vicinity reduces its sales growth. We propose a sequential demand-side pathway: shoplifting elevates shoppers' safety concerns (a psychological mechanism), reducing shopper traffic (a behavioral mechanism) and suppressing sales growth.

Next, applying routine activity theory (Cohen & Felson, 1979; Korgaonkar et al., 2021), we hypothesize that shoplifting's negative effect is weaker for (1) stores with higher *offline revenue share* (the proportion of a store's sales generated through in-person transactions)—which generates internal guardianship through heightened staff presence and physical monitoring, and (2) stores with greater *temporal agglomeration* (the degree to which shoppers' timings of visiting a store overlap with visit timings of neighboring stores)—which generates external guardianship through the co-presence of shoppers across neighboring stores during shared consumption windows.

The sample for our empirical analysis includes 495,973 shoplifting incidents across 27 cities from January 2019 to January 2026. The results confirm that shoplifting in a store's vicinity reduces its sales growth[1]. Mediation analysis indicates that reduced shopper traffic accounts for approximately 59% of shoplifting's total effect on sales growth, confirming that the primary pathway through which shoplifting suppresses sales is demand-side deterrence.

Our study makes three contributions. First, we provide causal evidence that quantitatively links shoplifting in a store's vicinity to suppressed store sales growth, a link previously undocumented by the literature, even though it carries substantial economic significance. At the median store in our sample, the average level of shoplifting is associated with a monthly sales loss of approximately 36% of median monthly sales, while the annualized aggregate loss across our sample exceeds $2.15 billion. Second, we propose the sequential pathway through which this effect manifests: shoplifting elevates shoppers' safety concerns (a psychological mechanism that is unobservable in our sample), followed by reduced shopper traffic (a behavioral mechanism that is observable in our sample), thereby impeding sales growth. Third, we identify offline revenue share and temporal agglomeration as the two store characteristics that explain the heterogeneity in this effect, extending routine activity theory to the context of retail store performance and providing retailers with actionable levers for mitigating shoplifting's financial consequences.

[1] We account for other local crime using counts of assault, drug offenses, fraud, homicide, robbery, sex crimes, weapons offenses, and a residual "other" category. We also account for sales level and prior sales growth.

LITERATURE ON BUSINESS EFFECTS OF SHOPLIFTING

Shoplifting is formally defined as incidents of theft, by someone other than a store's employee, of goods or merchandise exposed for sale within a retail store's vicinity (Federal Bureau of Investigation, 2011). Three themes across the literature (reviewed in Web Appendix A) hold relevance to our research: (1) shoplifting's costs to retailers (Hollinger & Davis, 2006), (2) the store-level responses to it (Kajalo & Lindblom, 2010), and (3) the effects on shoppers (Grewal et al., 2003).

Shoplifting's Costs for Retailers

Shoplifting is included under the category of theft. The annual security surveys of major U.S. retailers report theft as the most common cause of inventory shrinkage (Hollinger & Davis, 2006). The inventory shrinkage rates average approximately 1.7% of annual sales across major U.S. retailers (Hollinger & Davis, 2006), and these costs are borne largely internally, with only a fraction of apprehended shoplifters referred to law enforcement. Beyond direct inventory losses, shoplifting imposes indirect costs on retailers through loss-prevention investments that divert resources from revenue-generating activities. Shoplifting also imposes indirect costs on shoppers, such as retailers raising product prices to pass on a portion of shrinkage-related losses (Grewal et al., 2003) or the visible presence of loss-prevention infrastructure that can degrade the shopping experience, thereby suppressing patronage intentions (Baker et al., 2002).

Retailers' Responses to Shoplifting

Retailers respond to shoplifting through investments in (1) informal surveillance, such as incidental watching, and (2) formal surveillance, such as security cameras or alarm systems (Kajalo & Lindblom, 2010). These responses interact with the store environment, with physical cues specifically shaping shopper evaluations and behavior (Baker et al., 2002; Bitner, 1992;

Grewal et al., 2003). Concretely, these security measures can create a hostile shopping environment in which legitimate shoppers sense a lack of trust and perceive an invasion of their privacy. Shoppers' safety concerns are also shaped by information outside the store itself. Word of mouth, news, and social media coverage of local shoplifting incidents can lead shoppers to form negative safety perceptions of nearby stores without direct personal experience (Warr, 2000). Indeed, environmental cues associated with perceived area crime are significantly associated with reduced shopping intentions at retail districts (Baker et al., 2002).

Shoplifting's Effects on Shoppers

Shoplifting's effects extend to shoppers beyond those who are direct witnesses to shoplifting. Prior literature has shown that threat perceptions can dampen shoppers' evaluations of a store's atmosphere, reducing their visitation (Grewal et al., 2003). Shoppers' motivational orientations during their store visit may also shift from impulse purchasing to planned or deliberate purchasing (Puccinelli et al., 2009). Consequently, retail locations exposed to proximate safety threats experience measurable declines in shopper traffic and revenue (Chiong et al., 2025).

Research Gaps

Two important gaps remain in the literature. First, research has not examined whether, and if yes, why shoplifting translates into measurable consequences at the store level, a relationship that is theoretically and managerially significant. Its establishment would quantify shoplifting's economic impact beyond operational disruptions and provide retailers with an empirical basis for prioritizing loss-prevention investments. Similarly, evidence of the effect's pathway will help managers understand how the impact is channeled. Second, the conditions that explain the heterogeneity in these effects remain unexamined. Knowing that shoplifting harms store performance is only partially actionable; a complete understanding requires identifying the store-level conditions that mitigate this effect, equipping retailers with theoretically grounded levers for mitigation.

CONCEPTUAL FRAMEWORK

We propose that shoplifting in a store's vicinity impedes its sales growth, through a sequential process comprising two demand-side/shopper-specific variables: elevated shopper safety concerns and reduced shopper traffic.

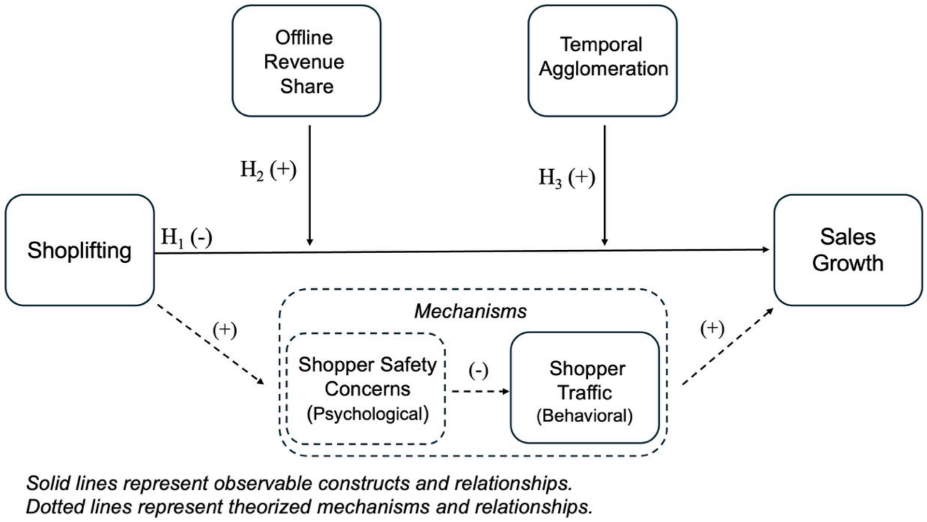
We draw upon routine activity theory (Bottoms & Wiles, 2020; Cohen & Felson, 1979) to examine conditions that can explain the heterogeneity in this effect. The theory posits that crime occurs when three elements converge in time and space: (1) a motivated offender, (2) a suitable target (e.g., a retail store), and (3) the absence of a capable "guardian". The theory takes its name from the observation that the routine activities of everyday life, such as work or shopping, structure the spatial and temporal patterns in which these three elements converge. It defines *guardianship* as persons' and/or environmental features' capacity to deter criminal activity through their presence alone, without requiring direct intervention (Reynald et al., 2018). These guardians may be "internal" to the space being guarded, such as habitual users or repeat patrons of a store, whose sustained presence creates an ongoing deterrent effect within the space itself. Complementarily, these guardians may be "external" to the space being guarded, such as bystanders and other individuals whose visibility signals that the space is occupied and under watch, thereby deterring potential offenders. Together, guardianship reduces a motivated offender's likelihood of acting on the opportunity presented by a suitable target (i.e., a retail store), thereby suppressing the safety concerns it generates among other users of the space (Hollis-Peel et al., 2011).

The routine activity theory helps us identify the following two guardians that can mitigate shoplifting's negative effect on store sales growth: (1) *offline revenue share* serving as an internal guardian, and (2) *temporal agglomeration* serving as an external guardian. Stores that derive greater revenue from in-person transactions have stronger incentives to invest in staff presence, store layout, and security infrastructure, signaling to shoppers that the premises are actively monitored and controlled and mitigating their safety concerns. Complementarily, concentrated shopper traffic during specific time windows creates natural surveillance through co-present shoppers, reducing the perceived opportunity for crime and mitigating shoppers' safety concerns. As safety concerns are mitigated, any reduction in shopper traffic is minimized, and shoplifting's negative effect on store sales growth is reduced. Table 1 defines our key constructs, and Figure 2 illustrates our conceptual framework.

Table 1: Key Constructs

Construct	*Definition*	*Representative References*
Sales growth	The rate at which a store's total sales expand or contract from one month to the next	Feng et al. (2017), Tuli et al. (2010), Whitler et al. (2018)
Shoplifting	Incidents of theft, by someone other than an employee of the victim, of goods or merchandise exposed for sale within a retail store's vicinity	Grewal et al. (2003), Hollinger & Davis (2006), Kajalo and Lindblom (2010)
Shopper safety concerns	A shopper's heightened perception of personal vulnerability and risk arising from awareness of nearby shoplifting activity	Baker et al. (2002), Chiong et al. (2025), Warr (2000)
Shopper traffic	The volume of distinct customers who made at least one purchase at a store in a given month	Chiong et al. (2025), Grewal et al. (2003)
Offline revenue share	The proportion of a retail store's revenue generated through in-person transactions, as opposed to online channels	Avery et al. (2012)
Temporal agglomeration	The overlap in consumption moments between the focal store and other stores co-located in its geographic area	Liu et al. (2018)

Figure 2: Conceptual Framework



HYPOTHESES

Shoplifting's Effect on Store Sales Growth

We hypothesize that shoplifting in a retail store's vicinity heightens shoppers' safety concerns, thereby reducing shopper traffic and impeding the store's sales growth. Next, we elaborate on this sequential pathway.

First, when shoplifting occurs near a retail store, accounts spread through social networks and local news. These accounts heighten shoppers' *safety concerns*, a psychological state in which shoppers perceive the store and its vicinity as unsafe (Warr, 2000). These concerns are not only cognitive assessments of risk, but also carry affective weight, generating anxiety that colors the entire anticipated shopping experience (Baker et al., 2002). National Retail Federation's Vice President for Asset Protection and Retail Operations, David Johnston, stated that "protecting store associates and customers, coupled with reducing today's levels of violence and retail crime, requires a whole-community approach and collaboration across all stakeholders" (National Retail Federation, 2024), implicitly recognizing that shoplifting creates the perception of an unsafe environment that extends beyond the store itself to the broader shopper community.

Second, heightened safety concerns lead shoppers to engage in spatial avoidance, staying away from locations they associate with a lack of personal safety (Chiong et al., 2025; Warr, 2000). This safety-induced anxiety redirects shoppers toward familiar, comfortable channels and away from environments perceived as unsafe (Puccinelli et al., 2009). The result is reduced shopper traffic to the store, as shoppers postpone trips, shorten them, or redirect patronage elsewhere.

Third, reduced shopper traffic at a store directly impedes sales growth. Each forgone visit represents a lost transaction, shrinking the pool of purchase opportunities available to the store (Grewal et al., 2003). Evidence from retail stores exposed to proximate safety threats confirms this mechanism: affected stores experience significant declines in shopper traffic, transactions, dwell time, and revenue (Chiong et al., 2025). Therefore, we predict:

H1: Shoplifting in a retail store's vicinity reduces the store's sales growth.

Focal Store's Offline Revenue Share as a Mitigator of Shoplifting's Negative Effect on Stores' Sales Growth

We hypothesize that a store's *offline revenue share*—the proportion of its total sales generated through in-person transactions—mitigates shoplifting's negative effect on store sales growth. We present two reasons for this hypothesis.

First, a store with a higher offline revenue share has stronger incentives to invest in staff density, store layout, and security infrastructure, since its product-market performance depends directly on sustaining in-store traffic. These investments constitute internal guardianship (Cohen & Felson, 1979). A store that is well-staffed and well-monitored signals active supervision, weakening the link between shoplifting and shoppers' safety concerns. Shoppers are less likely to interpret nearby shoplifting incidents as indicators of a lack of personal safety when visible guardianship competes with and dampens the threat signal that shoplifting would otherwise transmit.

Second, internal guardianship anchors shoppers' baseline perceptions of safety over time. Because stores with high offline revenue share maintain a consistently occupied and monitored environment, shoppers develop a stable reference point for the store as a safe, supervised space. Research on desensitization to crime finds that repeated exposure to an environment habituates individuals to its threat signals (Di Tella et al., 2019). Analogously, repeated exposure to a well-guarded environment stabilizes safety perceptions, making them resistant to upward revision in response to isolated incidents. Therefore, we predict:

> ***H2: The higher the focal retail store's offline revenue share, the weaker the shoplifting's negative effect on the store's sales growth.***

Focal Store's Temporal Agglomeration as a Mitigator of the Shoplifting's Negative Effect on Stores' Sales Growth

We hypothesize that a store's temporal agglomeration, the overlap in consumption moments it shares with other stores co-located in its geographic area (Liu et al., 2018), mitigates shoplifting's negative effect on store sales growth. We present two reasons for this hypothesis.

First, temporal agglomeration introduces *external* guardianship by increasing the density of co-present shoppers who can observe, intervene, or deter potential offenders (Cohen & Felson, 1979). Shoppers perceive that motivated offenders face a greater risk of detection and intervention in well-occupied environments, thereby reducing the extent to which shoplifting heightens perceptions of personal vulnerability. Further, the visible presence of an active crowd signals that the space remains under surveillance, competing with and dampening the threat signal that a shoplifting incident would otherwise convey, and limiting the extent to which shoppers revise their safety assessments downward.

Second, shoppers who do form safety concerns find those concerns less likely to suppress store visits under high temporal agglomeration. Dense active patronage serves as a persistent social cue that the environment remains safe and well-occupied (Grewal et al., 2003), generating favorable perceptions of the atmosphere that offset safety-based deterrence and sustain visit intentions despite nearby shoplifting activity. Therefore, we predict:

> ***H_3: The higher the focal retail store's temporal agglomeration with other stores in its vicinity, the weaker the shoplifting's negative effect on the store's sales growth.***

DATA AND METHODOLOGY

Data Sources

We assemble our estimation sample using multiple sources: SafeGraph (SafeGraph, 2022), municipal open data portals for 27 U.S. cities, and Advan Research. Our sample includes 2,383,801 store-month observations across 70,330 unique stores and is structured as a store × month panel spanning January 2019 through January 2026, covering 85 months.

Source for Store Sales Revenue Data

SafeGraph is a consumer spending dataset that aggregates anonymized credit and debit card transactions by store. We use it to collect store-month-specific sales revenue data, i.e., total sales, online sales, transaction count, and unique shopper count. We restrict the sample to branded retail stores: stores operating under a recognized brand name. SafeGraph assigns a unique identifier linking individual stores to their parent brand (SafeGraph, 2022). This restriction excludes independent single-location operators without a recognized brand affiliation, whose pricing, merchandising, and staffing practices are less standardized and exhibit low levels of online spending (Das & Mao, 2025). The retail sub-categories in our sample (listed in Web Appendix B) include limited-service restaurants, convenience stores, pharmacies, apparel retailers, and sporting goods stores.

Source for Shoplifting Data

We obtained shoplifting incident data from municipal open data portals across 27 cities.[2][3] These portals provide geocoded, incident-level records, including incident type (shoplifting, theft, assault, drug offenses, fraud, homicide, robbery, sex crimes, weapons offenses, and others), date, and block-level address of each reported crime (see Web Appendix C for the number of observations per city).

Source for Shopper Traffic Data

We use SafeGraph to capture online monthly sales and total sales for each store-month. We obtain data on temporal agglomeration by supplementing SafeGraph data with hourly traffic patterns from Advan Research's Weekly Patterns Plus dataset (Advan, 2025). This dataset reports weekly foot-traffic data derived from mobile device location signals, disaggregated by hour of the day for each store, yielding a 168-element (7 days × 24 hours) visit schedule.

Source for Covariates Data

SafeGraph Spend Patterns provides store-level sales and transaction variables, including total sales, lagged sales growth, shopper traffic, online sales, sales per transaction, online merchant count, and buy-now-pay-later provider count, as well as the category-level aggregates (category sales and category transactions), the Neighbors' Online Share measure, and all fixed effect identifiers (store, month, brand × month, and city × month). City open data portals provide

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[2] Chicago, Las Vegas, Dallas, Los Angeles, Charlotte, Fort Worth, Louisville, Raleigh, Nashville, Cincinnati, Memphis, Colorado Springs, Greensboro, Baltimore, Seattle, Kansas City, Baton Rouge, San Francisco, Chandler, Chattanooga, Rochester, Tempe, Gainesville, Detroit, Minneapolis, Johns Creek, and Burlington.

[3] We identify our city sample using the Live Crime Tracker maintained by NORC at the University of Chicago (Roman et al., 2025) (livecrimetracker.norc.org). We restrict the sample to the 27 of 50 cities in this tracker for which municipal open data portals provide geocoded, incident-level crime records with shoplifting coded as a distinct offense category, separate from broader theft or larceny classifications. Cities were excluded if they did not publish incident-level crime data publicly or reported only aggregate statistics rather than incident-level records.

data on non-theft crime covariate and both instrumental variables (See Table 2 for a summary of the measurement of our constructs and Web Appendix D for summary statistics of the variables).

Variables

Outcome Variable

Store Sales Growth (*Sales Growth*$_{i,m+1}$), is the natural log of store i's sales in month m + 1 divided by its sales in month *m* (Feng et al., 2017; Tuli et al., 2010; Whitler et al., 2018). Consistent with prior literature, we consider a log transformation because the raw sales growth exhibits severe departures from normality (skewness = 385.73, kurtosis = 204,353), indicating that the distribution is dominated by a small number of extreme values in the upper tail. The natural log transformation mitigates these concerns (skewness = .09, kurtosis = 13.15).

Explanatory Variable

The explanatory variable, Shoplifting (*Shoplifting*$_{i,m}$), is the natural log of one plus the number of shoplifting incidents reported within 5 miles of store i in month m, constructed from geocoded municipal crime records. The raw incident counts exhibit a zero-inflated, right-skewed distribution, with 25% of store-month observations recording zero incidents, a median of 19, and a mean of 46.57, driven upward by a long right tail concentrated among high-incident store-months (skewness = 1.89, kurtosis = 7.28). The log transformation substantially reduces this skewness, yielding a transformed variable (skewness = -.01, kurtosis = 1.25), consistent with a more symmetric distribution better suited for linear regression (Greene, 2012). Our transformation follows standard practice for count variables with heavily skewed distributions, reducing the influence of extreme values and improving the linearity of the relationship between the regressor and the outcome (Cameron & Trivedi, 2005; Wooldridge, 2010). Adding 1 before taking the log preserves the 25% of store-month observations recording zero incidents, which would otherwise be lost to the transformation.

Police records protect the privacy of involved individuals by providing only the block-level address for each shoplifting incident, rather than the exact location. We geocode these block-level addresses to latitude and longitude coordinates and use the resulting point locations to count monthly incidents within a 5-mile radius of each store (e.g., Guler, 2018). We later report robustness to alternative distance measures of 10, 15, and 20 miles.

Heterogeneity-Explaining Variables

Two store-month-level characteristics explain the heterogeneity in the relationship between shoplifting and sales growth. We lag both variables by one period to mitigate the concern that shoplifting may have affected their values. Thus, these characteristics reflect conditions *prior to* the shoplifting exposure.

First, Offline Revenue Share (*Offline Share*$_{i,m-1}$), is the ratio of the store *i*'s sales from shoppers' in-store spending to total sales in month $m - 1$ (Avery et al., 2012). Second, Temporal Agglomeration (*TA*$_{i,m-1}$), captures the degree to which a store's business hours overlap with those of nearby stores. Formally, for each store i in month $m-1$, let $\mathbf{v}_{i,m-1} \in \{0,1\}^{168}$denote the binary vector indicating the hours of the focal week in which the focal store receives non-zero shopper traffic, where the 168 elements correspond to 7 days × 24 hours. Temporal Agglomeration is defined as:

$$TA_{i,m-1} = \frac{|\ \mathcal{N}_{i,m-1}\ |}{\sum_{j\in\mathcal{N}_{i,m-1}} \sqrt{\sum_{h=1}^{168} (v_{ih,m-1} - v_{jh,m-1})^2}}$$

$\mathcal{N}_{i,m-1}$is the set of stores within 5 miles of the store i in the same city and month $m - 1$. $v_{ih,m-1}$equals 1 if store i receives non-zero shopper traffic in an hour h of month $m-1$ and 0 otherwise, and $v_{jh,m-1}$ is defined analogously for the neighboring store j. A higher value of $TA_{i,m-1}$ reflects the store's shopper traffic timing as being more like that of nearby stores, indicating greater overlap in consumption windows with the surrounding retail environment.

Covariates

Our regression includes three sets of store-level, category-level, and market-level covariates to isolate shoplifting's effect on store sales growth. First, we include store-level baseline characteristics. Total Sales ($Total\ Sales_{i,m}$) is the natural log of total sales at store i in month m, accounting for store size and baseline sales level and absorbing scale differences across stores (Tuli et al., 2010). Lagged Sales Growth ($Sales\ Growth_{i,m}$) is the natural log ratio of current-month to prior-month total sales, accounting for persistence in sales growth and accounting for dynamic adjustment in sales growth (Tuli et al., 2010). Shopper Traffic ($Traffic_{i,m}$) is the natural log of the number of unique shoppers visiting store i in month m, accounting for current-period patronage levels and separating shoplifting's effect on sales growth from baseline traffic differences (Sharma & Sharma, 2025).

Second, we account for the digital retail environment at the store level. Online Sales ($Online\ Sales_{i,m}$) is the natural log of online sales at store i in month m, accounting for the portion of sales generated through online transactions and isolating the offline competitive environment in which shoplifting operates (Avery et al., 2012). Neighbors' Online Share ($Neighbors'\ Online\ Share_{i,m}$) is the mean share of total sales attributable to online transactions for each store within 5 miles of store i in month m, accounting for area-level shifts toward digital purchasing that could independently reduce in-person traffic across all nearby stores (Avery et al., 2012). Online Merchant Count ($Online\ Merch_{i,m}$) is the number of distinct online merchants cross-shopped by store i shoppers in month m, accounting for the degree to which the store's shopper base is oriented toward online shopping and absorbing digital substitution effects unrelated to shoplifting (Avery et al., 2012). BNPL Providers ($BNPL_{i,m}$) is the number of distinct buy-now-pay-later providers used at store i in month m, accounting for fintech-driven purchase behavior that independently affects spending patterns at the store level (Kumar et al., 2024).

Third, we account for category-level and competitive market conditions. Category Sales ($Category\ Sales_{i,m}$) and Category Transactions ($Category\ Transactions_{i,m}$) are the mean log total sales and mean log number of transactions, respectively, across all stores in the same sub-category, city, and month m, absorbing category-level demand and traffic conditions common to stores in the same retail segment and local market (Trivedi et al., 2017). Sales per Transaction ($SPT_{i,m}$) is the ratio of total sales to the number of transactions at store i in month m, accounting for basket size differences across stores and months and separating sales effects driven by transaction frequency from those driven by transaction value (Trivedi et al., 2017). Non-theft Crime ($Non\text{-}theft\ Crime_{i,m}$) is the log ratio of all non-shoplifting, non-theft crime incidents to theft incidents within 5 miles of store i in month m, accounting for the broader local crime environment.

Lastly, we include store fixed effects and month fixed effects that absorb time-invariant store-level heterogeneity and common macroeconomic shocks, respectively, as well as brand × month fixed effects that absorb brand-level demand shocks and promotional activity common across all stores of the same brand in a given month. We also include city × month fixed effects that absorb local macroeconomic conditions and time-varying city-level shocks.

Table 2: Summary of Constructs and Definitions

Variable (Label)	***Measure***	***Data Sources***	***Reason for Inclusion, if Regressor***
	Outcome Variable		
Store Sales Growth *(Sales Growth$_{i,m+1}$)*	Natural log ratio of next-month (*m*+1) total sales to current-month (m) total sales for store *i*	SafeGraph Spend Patterns	NA
	Explanatory Variables		

Shoplifting *($Shoplifting_{i,m}$)*	Natural log of the number of shoplifting incidents within 5 miles of store *i* in month *m*	City Open Data Portal	NA
Offline Revenue Share *($Offline\ Share_{i,m-1}$)*	Ratio of total sales attributable to in-person spending to total spending for store *i* in month $m - 1$	SafeGraph Spend Patterns	Captures the degree to which a store's revenue depends on in-person shopping; represents the store’s shoppers as guardians
Temporal Agglomeration *($TA_{i,m-1}$)*	Inverse of the mean Euclidean distance between store *i*'s binary 168-hour shopper traffic visit schedule and those of all stores within 5 miles in the same city and month	Advan Weekly Patterns	Captures the degree to which the overlap in a store's shoppers’ traffic visit timing with that of nearby stores; represents other stores’ shoppers as guardians
	Mediator		
Shopper Traffic *($Traffic_{i,m}$)*	Natural log of the number of unique shoppers with at least one transaction visiting store *i* in month *m*	SafeGraph Spend Patterns	Accounts for current-period patronage levels, separating shoplifting’s effect on sales growth from baseline traffic differences
	Covariates		
Total Sales *($Total\ Sales_{i,m}$)*	Natural log of total sales at store i in month m	SafeGraph Spend Patterns	Accounts for store size and baseline Sales level, absorbing scale differences across stores
Lagged Sales Growth *($Sales\ Growth_{i,m}$)*	Natural log ratio of current-month to prior-month total sales for store *i*	SafeGraph Spend Patterns	Accounts for persistence in sales growth, accounting for dynamic adjustment in sales growth
Online Sales *($Online\ Sales_{i,m}$)*	Natural log of online sales at store *i* in month *m*	SafeGraph Spend Patterns	Accounts for Sales generated through online transactions, isolating the offline competitive environment in which shoplifting operates
Category Sales *($Category\ Sales_{i,m}$)*	Mean log of total sales across all stores in the same sub-category, city, and month *m*	SafeGraph Spend Patterns	Accounts for category-level demand conditions common to stores in the same retail segment and local market
Category Transactions *($Category\ Transactions_{i,m}$)*	Mean log of the number of transactions across all stores in the same sub-category, city, and month *m*	SafeGraph Spend Patterns	Accounts for category-level traffic conditions, separating store-specific effects from broader shifts in category patronage
Non-theft Crime *($Non\text{-}theft\ Crime_{i,m}$)*	Log ratio of all non-shoplifting, non-theft crime incidents to theft incidents within 5 miles of the store *i* in month *m*	City Open Data Portal	Accounts for the broader local crime environment, isolating the shoplifting’s effect on sales growth from general neighborhood crime conditions
Neighbors’ Online Share *($Neighbors'\ Online\ Share_{i,m}$)*	Mean share of total sales attributable to online transactions among stores within 5 miles of store *i* in month *m*	SafeGraph Spend Patterns	Accounts for the local online retail environment, absorbing area-level shifts toward digital purchasing that affect all nearby stores and could independently reduce in-person traffic
Sales per Transaction *($SPT_{i,m}$)*	Ratio of total sales to number of transactions at store *i* in month *m*	SafeGraph Spend Patterns	Accounts for basket size differences across stores and months, separating Sales effects driven by transaction frequency from those driven by transaction value
Online Merchant Count	Number of distinct online merchants cross-shopped by store *i*'s shoppers in month *m*	SafeGraph Spend Patterns	Accounts for the degree to which the store's shopper base is oriented toward

(Online Merch$_{i,m}$)			online shopping, absorbing digital substitution effects unrelated to shoplifting
BNPL Providers *(BNPL$_{i,m}$)*	Number of distinct buy-now-pay-later providers used at store *i* in month *m*	SafeGraph Spend Patterns	Accounts for fintech-driven purchase behavior that independently affects spending patterns at the store level
Store Fixed Effects	Binary indicators for each store *i*	SafeGraph Spend Patterns	Absorbs all time-invariant store-level heterogeneity, including location, format, and brand identity
Month Fixed Effects	Binary indicators for each year-month	SafeGraph Spend Patterns	Absorbs common macroeconomic shocks, seasonal demand patterns, and national retail trends
Brand x Month Fixed Effects	Binary indicators for each brand-year-month cell	SafeGraph Spend Patterns	Absorbs brand-level demand shocks and promotional activity common across all stores of the same brand in a given month
City x Month Fixed Effects	Binary indicators for each city-year-month cell	SafeGraph Spend Patterns	Absorbs local macroeconomic conditions and time-varying city-level shocks
	Instruments		
Distant Peer Homicide Rate *(Distant Peer Homicide$_{i,m-1}$)* *(Distant Peer Homicide$_{i,m-2}$)*	Mean homicide count among stores in other cities and other sub-categories, lagged one and two months	City Open Data Portal	Relevance: Homicide rates among stores in other cities and other sub-categories co-move with local shoplifting via a shared macro-level crime environment, thereby providing systematic variation in shoplifting exposure across retail categories and cities. Exclusion: Homicide activity in other cities and other sub-categories affects focal store Sales only through its correlation with local shoplifting intensity, with no plausible direct demand, competitive, or supply-side channel linking distant homicide patterns to sales growth.

Regression Specification and Estimation

We estimate the following equation to estimate shoplifting's effect on sales growth:

$$Sales\ Growth_{i,m+1} = \alpha + \beta_1\ Shoplifting_{i,m} + \beta_2\ Offline\ Share_{i,m-1} + \beta_3\ TA_{i,m-1} + \boldsymbol{X}_{i,m}\gamma + \mu_i + \lambda_m + \delta_{b(i),m} + \psi_{c(i),m} + \varepsilon_{i,m+1} \quad (1)$$

We augment Equation 1 to test the heterogeneity in the main effect:

$$Sales\ Growth_{i,m+1} = \alpha + \beta_1\ Shoplifting_{i,m} + \beta_2\ Offline\ Share_{i,m-1} + \beta_3\ TA_{i,m-1} + \beta_4\ (Shoplifting_{i,m} \times Offline\ Share_{i,m-1}) + \beta_5\ (Shoplifting_{i,m} \times TA_{i,m-1}) + \boldsymbol{X}_{i,m}\gamma + \mu_i + \lambda_m + \delta_{b(i),m} + \psi_{c(i),m} + \varepsilon_{i,m+1} \quad (2)$$

$\mathbf{X}_{i,m}$ is a vector of covariates, μ_i denotes store fixed effects, λ_m denotes month fixed effects, $\delta_{b(i),m}$ denotes brand × month fixed effects where *b(i)* indexes the brand of store *i*, $\psi_{c(i),m}$ denotes city × month fixed effects where *c(i)* indexes the city of store *i*, and $\varepsilon_{i,m}$ is the error term.

Endogeneity

Sources of Endogeneity

Exposure to shoplifting is unlikely to be randomly assigned across stores and months. Two sources of endogeneity are of particular concern. First, reverse causality may bias estimates if stores experiencing declining sales or traffic become more attractive targets for shoplifters, for instance, because reduced staffing levels lower the probability of detection and apprehension. Second, omitted-variable bias is a likely concern because unobserved local economic conditions, such as neighborhood poverty, unemployment, or social disorganization, may simultaneously increase shoplifting activity and reduce sales growth through channels unrelated to shoplifting itself. A store located in a high-crime, economically distressed neighborhood may experience lower sales and higher shoplifting for the same underlying reasons, inflating the apparent shoplifting's effect on sales growth.

Controlling for Endogeneity

We account for endogeneity in three ways. First, Third, we absorb time-invariant store-level heterogeneity through store fixed effects, which automatically accounts for omitted regressors such as store location quality, format, brand identity, and management practices that are stable over time and could otherwise confound the relationship between local shoplifting and store sales growth. Month fixed effects absorb common macroeconomic shocks, seasonal demand patterns, and national retail trends affecting all stores simultaneously. Brand × month fixed effects absorb brand-level demand shocks and promotional activity common across all stores of the same brand in a given month. City × month fixed effects absorb time-varying city-level confounders, including local macroeconomic fluctuations, changes in municipal policing intensity, and shifts in local retail market conditions that vary over time within a city and could simultaneously affect both shoplifting activity and store sales growth. Second, we estimate a lagged (rather than contemporaneous) regression. Specifically, we measure regressors in month m and the outcome in month $m + 1$, alleviating concerns about reverse causality. Third, we use instrumental variables to account for omitted-variable bias.

Instruments

We instrument Shoplifting$_{i,m}$ using the homicide rate of nearby "peers." Two points need qualification. First, we use homicide specifically rather than overall crime because homicide is the most consistently and accurately reported crime category across municipal police departments, making it a more reliable signal of the underlying crime environment than categories subject to reporting discretion, such as shoplifting itself (Blumstein & Rosenfeld, 1998). Second, "peers" are stores outside the focal store's city and sub-category. As an illustration, consider a Subway restaurant in Chicago in month m. Its distant peers are all stores that are simultaneously outside Chicago and outside the limited-service restaurant sub-category in that month, for example, a CVS Pharmacy in Dallas, or a Nike store in Seattle.

Instruments' Measures

Distant Peer Homicide Rate$_{i,m-1}$ and Distant Peer Homicide Rate$_{i,m-2}$, are both constructed from crime records across stores in other cities and retail sub-categories of the focal store (Shi et al., 2021). Formally, for each store i in sub-category s (e.g., limited-service restaurants) and city c, the instrument is the mean homicide count within 5 miles of stores that are simultaneously outside city c and outside sub-category s in month m (Goldsmith-Pinkham et al., 2020). We measure the instruments in months $m - 1$ and $m - 2$ to mitigate reverse causality concerns and ensure that the instruments predate the shoplifting exposure they are intended to predict.

Instruments' Relevance Criterion and Exclusion Restriction

The instruments satisfy the relevance criterion because crime environments co-move systematically across cities and retail categories through shared macroeconomic and social forces that simultaneously elevate shoplifting rates across geographically and categorically distant markets (Gupta et al., 2004). Homicide rates are particularly well-suited as a relevance signal because they are driven by the same underlying social disorganization that generates property crime, including shoplifting, yet are measured with far greater consistency and accuracy across municipal police departments than discretionary reporting categories such as shoplifting itself.

The exclusion restriction holds because homicide activity near stores in other cities and other retail sub-categories has no plausible direct pathway to the focal store's traffic or sales. Distant peer homicide rates cannot affect focal store sales growth through demand channels, as shoppers in other cities do not substitute their shopping across geographically distant markets. They cannot operate through competitive channels, as stores in different sub-categories do not compete for the same shoppers. They cannot operate through supply-side channels, as staffing, inventory, and merchandising decisions at the focal store are made locally and are insulated from crime conditions in unrelated markets. The instrument's variation, therefore, reaches focal store sales growth exclusively through its correlation with local shoplifting intensity, satisfying the exclusion restriction.

MAIN RESULTS

First-Stage Estimates

Web Appendix E reports first-stage regression estimates. The two excluded instruments, Distant Peer Homicide Rate lagged one and two months, are both positive and statistically significant regressors of shoplifting (b = .97, p = .000; b = .49, p = .004), confirming that homicide rates among stores in other cities and other retail sub-categories systematically co-move with local shoplifting activity through shared macroeconomic and social conditions driving crime environments. The positive signs are theoretically consistent: higher homicide rates in geographically and categorically distant markets signal a deteriorating broader crime environment that elevates shoplifting locally. Instrument strength is confirmed by multiple diagnostics. The Cragg-Donald Wald F statistic of 63.13 and the Sanderson-Windmeijer multivariate F statistic of 41.19 both substantially exceed the Stock-Yogo (2002) critical value of 19.93 for 10% maximal IV size, ruling out weak instrument concerns. The Kleibergen-Paap rk Wald F statistic of 41.19, which is robust to heteroskedasticity, likewise exceeds this threshold, providing further assurance that the instruments are sufficiently correlated with the endogenous regressor after partialling out all covariates and fixed effects. Instrument exogeneity is supported by the Hansen J overidentification test, which yields a statistic of 1.664 (p = .197), failing to reject the null hypothesis that the instruments are valid. Taken together, these diagnostics confirm that the Distant Peer Homicide Rate instruments are both strong and valid, satisfying the conditions necessary for consistent 2SLS estimation.

Model-Free Evidence for the Main Effect

We document the raw association between shoplifting and store sales growth. Figure 3A plots the mean next-month sales growth within each ventile (twenty equal-sized bins of the distribution) of the log shoplifting distribution. The negative slope of the OLS fit line indicates that stores in higher shoplifting environments systematically experience lower sales growth, with the relationship holding broadly across the full range of shoplifting exposure.

Figure 3A: Model-Free Evidence: Shoplifting and Sales Growth

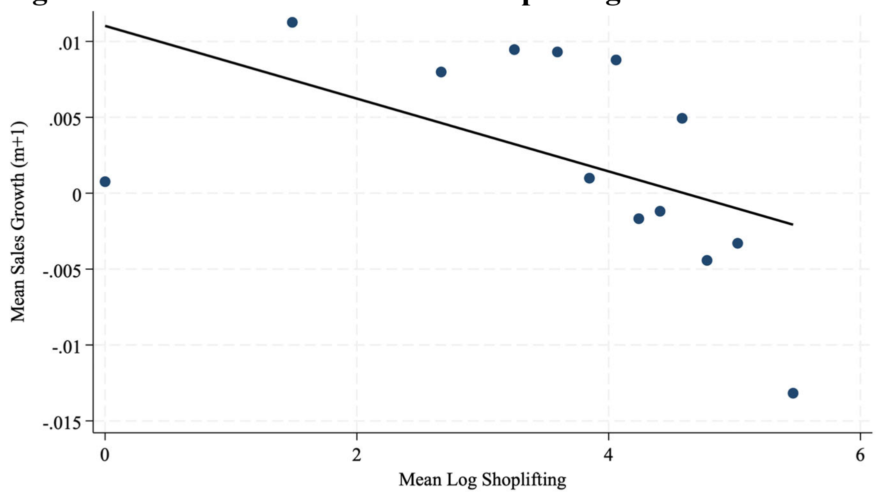
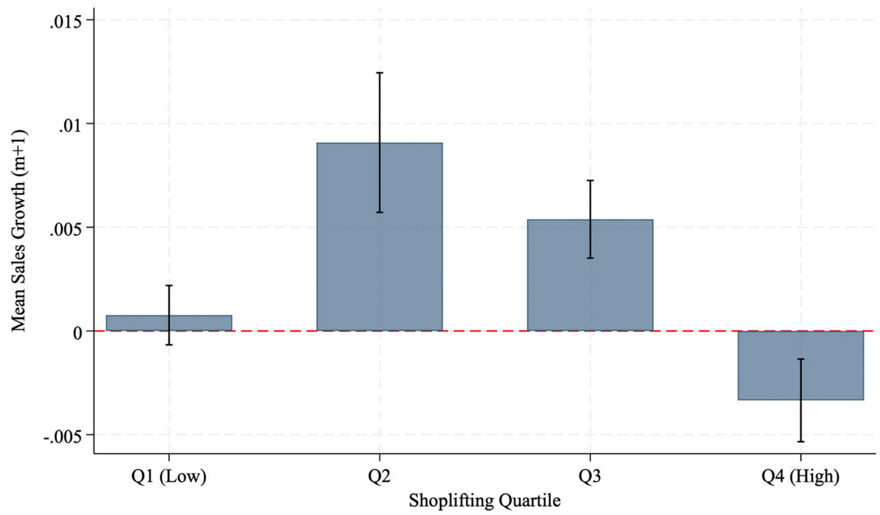


Figure 3B presents mean sales growth by quartile of the log-shoplifting distribution. The first quartile (Q1) recorded zero local shoplifting incidents and a mean monthly sales growth of 0.08%. Sales growth peaks in Q2 (mean = 11 incidents, sales growth = .91%), before declining monotonically through Q3 (mean = 46 incidents, sales growth = .54%) and turning negative in Q4 (mean = 137 incidents, sales growth = −.33%). A two-sample t-test comparing Q1 and Q4 confirms that the difference in mean sales growth of .41 percentage points is statistically significant (t = 3.33, p = .000). Taken together, the model-free evidence establishes a negative raw association between shoplifting and store sales growth.

Figure 3B: Sales Growth by Shoplifting Quartile



Second-Stage Estimates

Covariates' Associations

We report our estimation results in Table 3. Several covariates exhibit significant associations with store sales growth. First, store-level baseline characteristics show strong effects. Total Sales ($b = -.59$, $p = .000$) is negative, reflecting mean reversion in sales growth: stores with higher baseline sales levels tend to grow more slowly in the subsequent month, consistent with regression to the mean in sales trajectories. Lagged Sales Growth ($b = -.22$, $p =$ .000) is also negative, indicating that strong sales growth in the current month predicts weaker growth in the following month, further consistent with dynamic mean reversion in store-level sales. Shopper Traffic ($b = .38$, $p = .000$) is positive and significant, indicating that stores attracting more shoppers in the current month generate higher sales growth in the subsequent period, reflecting the forward-looking value of patronage.

Second, digital retail environment covariates exhibit significant effects. Online Sales ($b =$ -.00, $p = .000$) is negative, suggesting that higher online spending at a store is associated with lower subsequent sales growth, consistent with substitution from in-store to online purchasing channels that reduces physical retail momentum. Online Merchant Count ($b = -.03$, $p = .000$) and BNPL Providers ($b = -.02$, $p = .000$) are both negative, suggesting that stores whose shoppers are more digitally oriented and more reliant on alternative payment mechanisms tend to experience lower subsequent sales growth, consistent with these shopper profiles being associated with weaker in-store spending momentum. Sales per Transaction ($b = -.00$, $p = .005$) is also negative, indicating that stores with higher average basket sizes experience slightly lower subsequent sales growth, reflecting saturation in high-value transactions.

Third, category-level and competitive market conditions show expected patterns. Category Sales ($b = .01$, $p = .013$) and Category Transactions ($b = .05$, $p = .000$) are both positive, indicating that stores operating in categories experiencing broader demand growth and higher transaction volumes tend to exhibit stronger sales growth, capturing category-level effects. Non-theft Crime ($b = -.03$, $p = .000$) is negative, confirming that a higher ratio of violent and other serious crime to theft in the surrounding area independently suppresses store sales growth beyond the shoplifting channel, consistent with general neighborhood crime deterring retail patronage. Temporal Agglomeration ($b = -.49$, $p = .000$) is negative and significant, indicating that stores whose shoppers' traffic timing overlaps more closely with neighboring stores experience lower subsequent sales growth on average, consistent with greater competitive pressure among stores sharing the same consumption windows.

Main Effect

Turning to the focal variable, shoplifting exhibits a negative and statistically significant effect on store Sales Growth ($b = -.94$, $p = .000$), providing support for H_1. A one-unit increase in the log of local shoplifting incidents is associated with a .94 unit decline in the log ratio of next-month to current-month total sales, indicating that elevated shoplifting activity in the vicinity of a store meaningfully suppresses its forward-looking sales growth.

Heterogeneity in the Main Effect

Turning to the heterogeneous effects in column II, the interaction between Shoplifting and Offline Revenue Share (b = .68, p = .000) indicates that shoplifting's negative effect on sales growth is similarly mitigated for stores that derive a higher share of their revenue from in-person transactions, thereby supporting H_2. The interaction between Shoplifting and Temporal Agglomeration (b = .75, p = .005) indicates that shoplifting's negative effect on sales growth is weaker for stores whose shoppers' traffic timing overlaps more closely with neighboring stores and supports H_3.

Table 3: Estimation Results for Main Effect and Heterogeneity Effects

	Column I			*Column II*		
Outcome Variable: Sales growth (m+1)	*Model 1: Main Effect*			*Model 2: Heterogeneous Effects*		
Variable	*b*	*SE*	*p*	*b*	*SE*	*p*
Covariates						
Total Sales	-.59	.00	.000	-.59	.00	.000
Sales Growth (m-1)	-.22	.00	.000	-.22	.00	.000
Traffic	.38	.00	.000	.38	.00	.000
Online Sales	-.00	.00	.000	-.00	.00	.000
Category Sales	.01	.01	.013	.01	.01	.049
Category Transactions	.05	.01	.000	.05	.01	.000
Non-theft Crime	-.03	.01	.000	-.04	.01	.000
Neighbors' Online Share	.08	.10	.429	.01	.09	.939
SPT	-.00	.00	.005	-.00	.00	.006
Online Merch	-.03	.00	.000	-.03	.00	.000
BNPL	-.02	.00	.000	-.02	.00	.000
Explanatory Variable						
Shoplifting	-.94	.21	.000	-1.77	.32	.000
Heterogeneity-Explaining Variables						
Offline Share (m-1)	.02	.01	.147	-1.56	.41	.000
TA (m-1)	-.49	.12	.000	-1.86	.52	.000
Interactions						
Shoplifting x Offline Share (m-1)				.68	.18	.000
Shoplifting x TA (m-1)				.75	.27	.005
Store, Month, Brand x Month, City x Month	Yes					
Diagnostics						
Observations	1,905,826[4]			1,905,770		

[4] Our estimation sample includes 2,383,801 store-month observations. Specifically, 420,748 of these observations are lost due to missing values on regression variables, primarily because constructing the second-lag instrument, lagged moderators, and forward sales growth outcome requires at least two consecutive months of panel data for each store. An additional 57,224 singleton observations are dropped by the estimator as they contribute no

Clustering level	Store x month			
Cragg-Donald Wald F statistic	63.13		36.84	
Kleibergen-Paap rk Wald F statistic	41.19		23.81	
Sanderson-Windmeijer F test	41.19	.000	51.78, 154.32, 28.18	
Hansen J statistic	1.664	.197	1.797	.180
Stock-Yogo critical value (10%)	19.93		N/A	

Mediation Analysis: Does Shopper Traffic Explain Shoplifting's Effect on Sales Growth?

We test the mediating role of shopper traffic using the instrumental-variables mediation framework proposed by Dippel et al. (2020), which is designed for settings in which both the treatment variable and the mediator are endogenous. Consistent with Dippel et al. (2020), we estimate three separate 2SLS regressions with high-dimensional fixed effects absorbed at the store, month, brand × month, and city × month levels, and standard errors robust to heteroskedasticity.

The three regressions estimate (I) shoplifting's effect on shopper traffic (the α path, instrumented by the Distant Peer Homicide Rate lagged one and two months); (II) the total effect (τ) of shoplifting on sales growth (instrumented by the same set); and (III) shoplifting's direct effect on sales growth conditional on the instrumented mediator (shopper traffic), from which both the direct effect (the δ path) and shopper traffic's effect on sales growth (the β path) are recovered, with both shoplifting and shopper traffic treated as endogenous. The mediator (shopper traffic) instrument is the average log traffic across stores in other cities and retail sub-categories. The indirect effect is recovered by two internally consistent methods: (1) as the difference $\tau - \delta$ (total effect minus direct effect), and (2) as the product $\alpha \times \beta$ (α path times β path). Convergence of these two estimates provides a model consistency check (Dippel et al., 2020). We use the Sobel test to assess the indirect effect's significance.

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identifying variation within their fixed effect groups, yielding a final estimation sample of 1,905,826 observations for the main effect model and 1,905,770 for the moderation model.

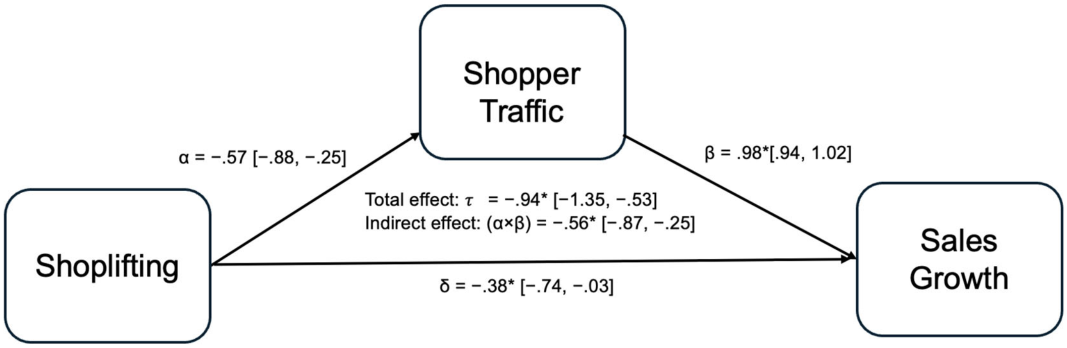
Table 4 and Figure 4 present the mediation results. The α path shows that shoplifting significantly reduces shopper traffic ($b = -.57$, CI [−.88, −.25]), confirming that elevated local shoplifting lowers shopper traffic. The β path shows that shopper traffic significantly increases sales growth ($b = .98$, CI [.94, 1.02]), reflecting the central role of in-store patronage in driving sales growth. Together, these two pathways produce a negative and statistically significant indirect effect ($\alpha \times \beta = -.56$, CI [-.87, -.25]), with the confidence interval excluding zero and confirming that shopper traffic deterrence is a meaningful mediating mechanism. The close convergence of the two indirect effect estimates ($\tau - \delta = -.56$ and $\alpha \times \beta = -.56$) confirms the internal consistency of the mediation decomposition.

Shoplifting's direct effect on sales growth remains negative and statistically significant after accounting for the shopper traffic channel ($b = -.38$, CI [−.74, −.03]), indicating that shoplifting also damages sales through mechanisms beyond traffic deterrence. Shoplifting's total effect on sales growth is −.94 (CI [−1.35, −.53]). The indirect pathway accounts for 59.14% of the total effect, while the direct pathway accounts for the remaining 40.86%, indicating partial mediation. Therefore, the shopper traffic deterrence explains how shoplifting suppresses store sales growth, with nearly three-fifths of the total sales damage attributable to reduced shopper traffic to affected stores.

Table 4: Mediation Analysis: Shoplifting's Effect on Sales Growth via Shopper Traffic

	b	*SE*	*95% CI*	*% of Total*
Shoplifting → Shopper Traffic (α path)	-.57	.16	[-.88, -.25]	
Shopper Traffic → Sales growth (β path)	.98	.02	[.94, 1.02]	
Indirect effect ($\tau - \delta$)	-.56	.16	[-.87, -.25]	59.14%
Indirect effect ($\alpha \times \beta$)	-.56	.16	[-.87, -.25]	59.14%
Direct effect (δ): Shoplifting → Sales growth	-.38	.18	[-.74, -.03]	40.86%
Total effect (τ): Shoplifting → Sales growth	-.94	.21	[-1.35, -.53]	100.00%

Figure 4: Mediation framework



ROBUSTNESS AND ADDITIONAL ANALYSES

Robustness to Alternative Radii

Our main analysis operationalized a store's vicinity by 5 miles. Next, we assess the main effect's robustness by re-estimating all models using crime measured at 10-, 15-, and 20-mile radii around each focal store. Shoplifting's negative effect on sales growth remains statistically significant and directionally consistent across all three specifications (see Table 5 for results). The 10-mile estimate ($b = -2.40$, $p = .000$) is comparable in magnitude to the primary 5-mile specification, while the 15-mile estimate ($b = -4.49$, $p = .000$) and 20-mile estimate ($b = -19.13$, $p = .000$) grow larger in absolute terms, consistent with shoplifting measured over wider geographic windows capturing greater cumulative crime exposure that lowers sales growth.

Furthermore, the heterogeneity (explained by Offline Revenue Share and Temporal Agglomeration) in the main effect replicates across all three radii. Offline revenue share mitigates shoplifting's negative effect on sales growth ($b_{10\text{-}mile} = .45$, $p = .000$; $b_{15\text{-}mile} = .41$, $p = .000$; and $b_{20\text{-}mile} = .43$, $p = .000$). The focal store's temporal agglomeration with its neighboring stores consistently mitigates the decline in sales growth ($b_{10\text{-}mile} = 1.17$, $p = .000$; $b_{15\text{-}mile} = .75$, $p = .002$; and $b_{20\text{-}mile} = 1.00$, $p = .000$ at 10, 15, and 20 miles, respectively). Collectively, these results indicate that the findings are not an artifact of the spatial radius chosen to define local crime exposure.

Table 5: Estimation Results for Main Effect and Heterogeneity Effects at Alternative Crime Radii

	10-Mile			*15-Mile*			*20-Mile*		
Main Effect	*b*	*SE*	*p*	*b*	*SE*	*p*	*b*	*SE*	*p*
Shoplifting	−2.40	.59	.000	−4.49	1.01	.000	−19.13	4.42	.000
Heterogeneous Effects									
Shoplifting × Offline Revenue Share	.45	.11	.000	.41	.10	.000	.43	.11	.000
Shoplifting × Temporal Agglomeration	1.17	.29	.000	.75	.24	.002	1.00	.28	.000

Standardized Coefficients

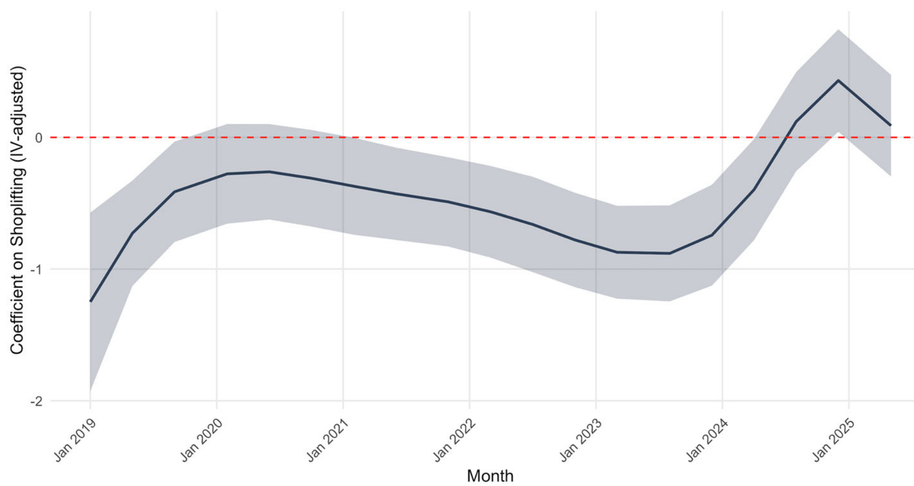
Next, we aim to *compare* the estimated coefficients of regressors of theoretical interest and thus draw intuitive insights. Therefore, we re-estimate the main-effect and heterogeneous-effects regressions using standardized variables, allowing the coefficients to be interpreted in standard deviation units. Shoplifting's negative effect on sales growth remains statistically significant (b = −2.80, p = .000), indicating that a one-standard-deviation increase in shoplifting in a month is associated with a 2.80 standard deviation decline in sales growth in the following month. Likewise, the two heterogeneous effects replicate. The interaction effect coefficients of offline revenue share (b = 2.01, p = .000) and temporal agglomeration (b = .30, p = .006) are positive and significant.

Time-Varying Effects Model

Next, we assess whether shoplifting's negative effect on store sales growth is stable across the study period or varies systematically over time. Empirically, we estimate a time-varying effects model (TVEM) following Shiyko et al. (2012). Rather than constraining the shoplifting coefficient to a single pooled estimate, the TVEM estimates a smooth coefficient function $\beta(t)$ that captures how the relationship between shoplifting and sales growth evolves across the 85-month panel (Dziak et al., 2020).

Figure 5 presents shoplifting's estimated coefficient on sales growth from January 2019 through January 2025. We trimmed the last months of the panel to avoid spline boundary artifacts. Shoplifting's coefficient on sales growth is negative and statistically significant for most of the sample months, with the confidence band excluding zero across much of the time window. The effect is strongest in magnitude in early 2019, mitigates toward zero during the COVID-19 lockdown period of mid-2020 to early 2021, when in-person retail activity collapsed, and the shoplifting-to-sales growth relationship was mechanically disrupted, and deepens again through 2022-2023. The TVEM estimates suggest that shoplifting's negative effect on sales growth is a persistent feature of the retail environment across the full study period rather than an artifact of a particular macroeconomic episode.

Figure 5: Shoplifting's Time-Varying Effect on Sales Growth



Spotlight and Floodlight Analysis

To assess the boundary conditions of the shoplifting-sales growth relationship, we conduct spotlight and floodlight analyses following Spiller et al. (2013). Spotlight analysis evaluates the effect of shoplifting at theoretically meaningful values of each heterogeneity explainer (mean and mean ± 2SD), while floodlight analysis traces the simple continuously across the full observed range, identifying where the effect transitions from significant to non-significant (Spiller et al., 2013). Figure 6A and 6B present results for offline revenue share and temporal agglomeration, respectively. Shoplifting's negative effect on sales growth remains significant across all values of offline revenue share within the mean ± 2SD range, confirming that the effect is not confined to any particular segment. Consistent with H_2, stores with low offline revenue share suffer greater losses. Similarly, shoplifting's negative effect on sales growth stays significant at all values of temporal agglomeration within the mean ± 2SD range. Stores in temporally synchronized retail clusters experience smaller losses than those in desynchronized clusters, consistent with H_3. Across both heterogeneity explainers, the confidence band excludes zero throughout the observed range, confirming that shoplifting's negative effect on sales growth is robust rather than localized.

Figure 6A: Floodlight and Spotlight Analysis: Offline Revenue Share

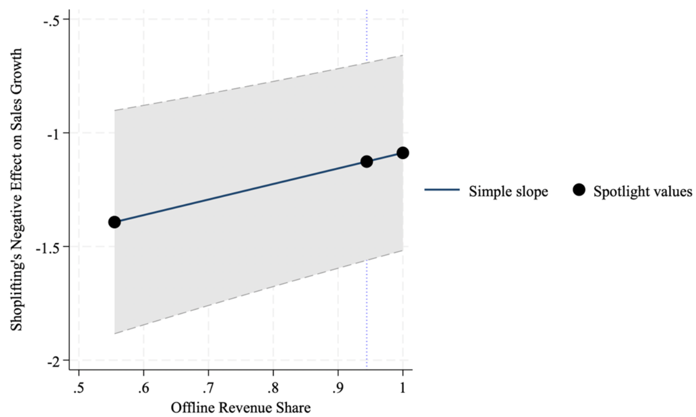
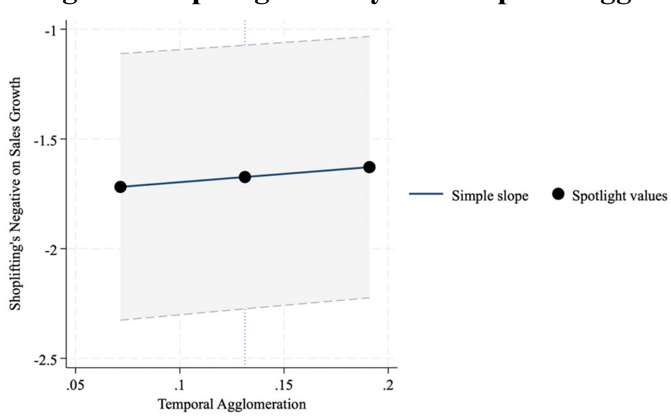


Figure 6B: Floodlight and Spotlight Analysis: Temporal Agglomeration



Economic Significance

To assess economic significance, we re-estimate the main model, replacing the log-transformed shoplifting measure with the raw incident count, allowing the coefficient to reflect the effect of each additional incident reported within a 5-mile radius of the store in any month (b = -.02, p = .000). Benchmarking at the median store monthly sales of $2,094, ten additional shoplifting incidents within a 5-mile radius are associated with a 19-percentage-point decline in sales growth and $397 monthly sales loss. At the median shoplifting level of 19 incidents, the estimated monthly loss is $754.25 (annualized loss is $9,051), equivalent to approximately 36% of the median monthly sales. Moving a store from the 25th (0 incidents) to the 75th percentile (74 incidents) of the shoplifting distribution is associated with a monthly sales loss of $2,938.73 (i.e., 140 percent of the median monthly sales) and an annualized loss of $35,253.72. Stores at the 90th percentile (132 incidents) incur an annualized sales loss of $62,877.60 (roughly 2.5 times the median annual store sales). Aggregating across all 70,330 stores in our sample, a one-standard-deviation increase in shoplifting (64 incidents) implies an annualized sales loss of $2.15 billion ($30,522 per store annualized), underscoring the substantial economic burden that elevated retail crime imposes on the broader retail sector.

Combining Instrumental Variables with Copula Correction

Our instrumental-variables strategy satisfies the relevance and exclusion criteria, but no instrument-based approach can fully rule out residual endogeneity. We therefore supplement the IV strategy with copula correction, following the 2sCOPE procedure of Yang, Qian, and Xie (2025). For each model, we compute the copula-transformed shoplifting term and regress it on the copula-transformed exogenous regressors. The residual from this first-stage model serves as a control function, which we add as a covariate alongside the IV in the structural model. A significant coefficient on this term signals residual endogeneity beyond what the instruments correct, and its inclusion only strengthens identification rather than weakens it. The main effect of shoplifting on sales growth remains negative and significant after this correction (b = −1.74, p = .000), and the copula term itself is significant (b = 2.14, p = .000), confirming that the correction captures meaningful residual dependence. The two moderating effects also hold under copula correction: shoplifting's interaction with temporal agglomeration (b = 2.09, p = .000) and with offline revenue share (b = .93, p = .000) both remain positive and significant, mirroring the pattern from the IV-only specification in Table 3. Combined with a significant copula term in this model as well (b = 1.93, p = .000), these results confirm that our main and moderating effects are not an artifact of residual regressor-error dependence left uncorrected by the instruments alone.

Robustness to Selection on Baseline Shoplifting Exposure

A further concern is that stores exposed to shoplifting early in the panel may differ systematically from stores that are not, in ways that drive both later shoplifting and sales growth. We address this concern with propensity score matching. For each store, we classify it as exposed if it records any shoplifting incidents within its first six months in the panel, and unexposed otherwise. We estimate the propensity to be exposed using a logit model on pre-period store characteristics (log customers, log online spending, and log total spending), then match each exposed store to its nearest unexposed neighbor on the propensity score within a caliper of .05. This procedure matches 33,426 exposed stores to 16,176 unique unexposed stores, yielding a matched sample of 49,602 stores and all exposed stores matched within caliper. We re-estimate the main IV model and the moderation IV model on this matched panel, applying match weights. Shoplifting's negative effect on sales growth remains negative and significant in the matched sample (b = −1.38, p = .000), and the two moderating effects also replicate: the interaction with temporal agglomeration (b = 1.16, p = .000) and the interaction with offline revenue share (b = .70, p = .000) both remain positive and significant. These results confirm that our findings are not driven by systematic pre-period differences between stores with and without early shoplifting exposure.

Robustness to an Alternative, Within-City Instrument

Our primary instrument draws on homicide rates among distant peer stores. As an alternative, we construct a within-city instrument that interacts local theft crime with the lagged share of female police officers in the same city. This instrument trades some of the exclusion restriction's strength, since local crime conditions are more plausibly linked to local store outcomes through channels other than shoplifting, for substantially greater relevance, since same-city crime conditions predict local shoplifting far more strongly than conditions among distant peers do. We re-estimate the main and moderation models using this instrument, absorbing store and month fixed effects. The first stage confirms the gain in relevance: the Cragg-Donald Wald F statistic reaches 405,019.5 in the main-effect model and 1,761.89 in the moderation model, both far exceeding the corresponding figures of 63.13 and 36.84 from our main specification in Table 3. Shoplifting's negative effect on sales growth remains negative and significant (b = −.02, p = .000), and the two moderating effects also replicate: the interaction with offline revenue share (b = .12, p = .001) and the interaction with temporal agglomeration (b = .65, p = .000) both remain positive and significant.

ALTERNATIVE EXPLANATIONS

Supply Chain Disruption

One alternative is that shoplifting-induced inventory shrinkage triggers supply chain shocks that reduce product availability and suppress store sales independently of shopper demand (Hollinger & Davis, 2006). If operative, this mechanism should also reduce online sales, since supply disruptions would affect the store's entire product offering across channels. We test this by re-estimating the main model with next-month online sales as the outcome. Shoplifting's coefficient is statistically indistinguishable from zero (b = .01, p = .996), indicating no detectable effect on online spending, inconsistent with a supply-side mechanism.

Shopper Spending

A second alternative is that shoplifting suppresses basket size (i.e., spending per shopper) rather than shopper traffic. This mechanism implies that shopper traffic remains unchanged, but spend per trip declines, perhaps because surveillance measures create an unpleasant shopping environment, reduce assortment depth, or prompt removal of high-value items from display. We test this plausibility by re-estimating the main model with next-month spend per shopper as the outcome. Shoplifting's coefficient is insignificant (b = 792.34, p = .207), providing no evidence that shoplifting reduces average basket size, consistent with shopper traffic, rather than per-shopper spending, as the primary mechanism.

Store Exit

The third alternative is store exit: shoplifting may cause permanent store closures (McArdle, 2022), generating a negative association between shoplifting and sales that reflects exit rather than declining performance among surviving stores. We test this by re-estimating the main model with an indicator for next month's store closure as the outcome. Shoplifting's coefficient is insignificant (b = .01, p = .295), indicating that elevated local shoplifting does not significantly increase the store closure probability. The main effect thus reflects genuine sales decline among surviving stores rather than a compositional artifact driven by shoplifting-induced exit.

DISCUSSION

Shoplifting costs U.S. retailers dearly, yet the discipline has treated it primarily as an operational problem, cataloging shrinkage rates and loss-prevention tactics while leaving its product-market consequences unexamined. This study reframes shoplifting as a demand-management challenge. Our findings establish that shoplifting causally suppresses sales growth of nearby stores through a sequential pathway from shoppers' safety concerns to reduced shopper traffic, and that offline revenue share and temporal agglomeration mitigate this pathway by generating internal and external guardianship, respectively. Together, these results reveal that the performance consequences of shoplifting are considerably more far-reaching than the operational cost literature has recognized.

Contributions to Theory

Literature on Shoplifting and Retail Performance

Prior research has established that shoplifting imposes direct operational costs via inventory shrinkage (Hollinger & Davis, 2006) and that shoplifting-related cues shape shopper evaluations and in-store behavior (Baker et al., 2002; Grewal et al., 2003). What has remained unexamined is whether these shopper-level responses aggregate into measurable, store-level consequences. We establish that they do so: shoplifting reduces store sales growth through a demand-side pathway in which shoppers' heightened safety concerns reduce shopper traffic, thereby suppressing sales growth. This sequential pathway connects a criminological phenomenon to a business performance outcome, offering a more complete account of the consequences of shoplifting than operational cost frameworks alone provide.

This contribution is non-trivial for two reasons. First, shoplifting differs fundamentally from the acute crime events whose retail consequences have recently been documented. Chiong et al. (2025) established that mass shootings near retail locations suppress store revenue and foot traffic through demand-side channels, a foundational finding that our study extends to a qualitatively different crime type. Shoplifting is chronic, ubiquitous, and low-salience rather than discrete and visceral. Its signals reach shoppers through environmental cues, word of mouth, and media coverage, and individual incidents are too routine to command the psychological urgency of a violent event. Whether such a diffuse, normalized crime signal accumulates into economically meaningful store-level consequences is genuinely uncertain and cannot be resolved by theory alone. Second, shoplifting's high base rate means that even modest per-incident effects, compounded across thousands of store-months, can produce aggregate losses of considerable magnitude, making a chronic, taken-for-granted feature of the retail environment both theoretically non-intuitive and practically significant.

Literature on Store Characteristics that Mitigate the Negative Effect of Shoplifting

Prior research on retail performance heterogeneity has focused on competitive structure, store format, and brand equity as moderators, leaving the role of guardianship-based characteristics largely unexplored. Drawing on routine activity theory, we show that offline revenue share (by generating internal guardianship through heightened incentives for staff and monitoring of the physical environment) and temporal agglomeration (by generating external guardianship through the natural surveillance of co-present shoppers) each weaken the pathway from shoplifting to reduced sales growth. These findings reframe both constructs not merely as demand-side characteristics but as governance-adjacent characteristics that shape a store's resilience to retail crime, adding a crime-environment dimension to the retail performance literature that has not previously been articulated.

Routine Activity Theory

Developed to explain crime incidence, the routine activity theory has remained largely within that domain (Cohen & Felson, 1979; Hollis-Peel et al., 2011). We extend it to the intersection of retail crime, a social nuisance, and business performance, showing that guardianship mechanisms operate not only on the likelihood of criminal acts but also on the downstream psychological and behavioral responses those acts generate among shoppers. Guardianship does not merely deter offenders. It also stabilizes shoppers' perceptions of safety, thereby preserving the commercial viability of the retail environment. This extension broadens routine activity theory's explanatory scope and positions it as a useful theoretical lens for marketing scholars examining how environmental conditions shape consumer behavior and firm performance in the presence of retail crime.

Implications for Practice

Our findings offer three implications for practice. First, our evidence suggests an additional criterion for evaluating loss-prevention investments: their capacity to signal guardianship to shoppers and to preserve shopper traffic. Conventional investments (such as security cameras and alarm systems) are typically assessed solely on their deterrence effectiveness. Our results indicate that visibility matters equally. Investments that communicate safety to legitimate shoppers (such as well-staffed service areas and open store layouts) may yield greater returns on shopper retention than equivalent investments in formal surveillance infrastructure that shoppers may perceive as threatening rather than reassuring.

Second, retailers with a higher share of offline revenue face greater exposure to shoplifting's effects, since more of their revenue depends on in-store traffic. Yet, our findings show that a higher offline revenue share also generates internal guardianship that mitigates this very effect. Retailers weighing channel mix decisions (such as expanding e-commerce versus maintaining physical retail), should account for this guardianship externality. A sustained commitment to in-store revenue is not only a channel strategy but also a crime-resilience strategy, as the staffing and monitoring it requires buffers stores against the demand-side consequences of shoplifting.

Third, retailers selecting new store locations or evaluating co-tenancy arrangements typically focus on demand complementarity and competitive overlap. Our results add a guardianship consideration: stores that co-locate with neighbors whose traffic periods overlap with their own benefit from a denser external guardian population, reducing the extent to which nearby shoplifting elevates shoppers' safety concerns. Retail chains and shopping center developers can use this logic to design co-tenancy arrangements that generate mutual guardianship benefits alongside demand spillovers.

Limitations and Future Research Directions

First, our sample includes retail stores in 27 U.S. cities, limiting the generalizability of our findings to independent retailers and non-U.S. contexts. Independent retailers lack the brand equity, loss-prevention infrastructure, and organizational resources of chain retailers, and shoppers' safety-concern formation may operate differently in those settings. Future research should examine whether the pathway and guardianship mechanisms we document replicate in independent retail contexts and in international markets where the relationship between crime, safety perception, and shopping behavior may be shaped by different cultural and institutional conditions. Second, shoppers' safety concerns, the psychological mediator central to our framework, are unobserved in our observational dataset. Future research using experience sampling methods or in-store physiological measurements could directly capture the affective and cognitive states that link shoplifting exposure and behavioral response, providing a richer account of the mechanism than panel data alone can provide. Third, the guardianship framework suggests that the identity and behavior of co-present shoppers, rather than their mere density, may shape safety concerns. Future research could examine whether guardian characteristics such as familiarity, perceived authority, or attentiveness extend routine activity theory from a presence-based to a quality-based account, with direct implications for store staffing and community design. Fourth, our study treats shoplifting as a condition that stores respond to but do not shape. In practice, retailers make active choices about how to communicate their loss prevention posture to shoppers through store design, staff training, and public-facing security investments. Future research could examine how retailers' communication of their loss-prevention efforts shapes shoppers' safety concerns, independent of actual shoplifting incidence, potentially identifying messaging strategies that preserve shopper traffic even in high-crime environments.

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